Biogeosciences Discuss., 9, C1840–C1849, 2012
www.biogeosciences-discuss.net/9/C1840/2012/

Biogeosciences
Discussions

Interactive comment on "High-resolution interpolar difference of atmospheric methane around the Last Glacial Maximum" *by* M. Baumgartner et al.

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Received and published: 9 June 2012

General Comments

This paper is an excellent addition to the scientific literature and is within the scope of Biogeosciences. It presents new data that leads to an important revision of our understanding of the methane interpolar difference (IPD) during the last glacial termination. The authors have in general presented a very high quality discussion of the IPD, but I do have some comments/suggestions about a number of topics including the specifics of their modeling choices, their logic behind the connection between speleothem records and their IPD record, and a few other topics. I applaud the authors for their hard work

to produce this record and look forward to seeing a revised manuscript.

Specific Comments

Page 5473; line 4. The rIPD in 2010 is about 7% and later in the paper (pgs 5487, 5492) this is referred to as the "present day anthropogenicly modified rIPD (7 %)". However, if you look at the rIPD from the northernmost and southernmost NOAA sites for the whole modern record you can see that the rIPD was much higher (∼8.5%) in the 1980s and has decreased since then. I calculate that the average rIPD over the whole modern record is 7.6%. I think this would be a better figure to report than just 2010 which is actually one of the lower points in the modern record. Perhaps it would be good to also report the standard deviation (0.5%) on this as well, just to give readers a reference as to the amount that the rIPD can change on an interannual basis. I would encourage the authors to verify my calculations.

Page 5473; line 29. Levine et al (2011) does not find that the effects of BVOCs are negligible. They find that the effect of BVOCs is counteracted by the effects of temperature and that the net change after accounting for the effects of temperature change and BVOCs is negligible.

Page 5474; line 21-22. Technically this statement "…thus represents about the maximum resolution possible." Is correct, but it is misleading since higher resolution and/or higher precision measurements would reduce the uncertainties of the IPD. My opinion is that this statement should just be removed because it does not add anything.

Page 5475; line 7. This is not an entirely circular argument because atmospheric smoothing and firn air smoothing would cause the fast and strong variations in CH4 to be smoothed out. If a true atmospheric variation in the IPD lasted for long enough to actually be visible in the ice core record you would need to invoke some extreme scenarios such as shutting down NH sources for multiple years, possibly a decade. This is such an extremely unlikely scenario that it can be ignored based on Occam's razor.

Page 5476; Line 3. While the effects of gravitational depletion may not affect the IPD very much, they do have an impact on the absolute concentration values. Since this is a known effect and the authors state that it is a simple correction, I see no reason NOT to make the correction. If the correction is not made, it becomes cumbersome for other workers to compare their data to this study and thus limits the utility of their data. It could also increase the confusion among people who are trying to use their data but do not have specialized knowledge about all the specific corrections that ice core records need. If the authors feel that their estimate of gravitational depletion is not sufficiently accurate to make the correction, then this should be explained in detail.

Also, as a smaller point, I don't understand how they calculated the combined IPD error on line 1 (0.5 +/- 0.7 ppb). I see that 2.9-2.4 = 0.5, but if you take the max and min uncertainties you should get 0.5 +/- 1.0 ppb, right?

Also, they do not attempt to correct for the effects of solubility or even mention this. Since they are using a wet extraction technique they should address this topic as in Mitchell et al (2011). Again, if their results are to be compared with other labs, or with data obtained from a dry extraction technique, they should correct for this.

As a final point about the methods, I did not see anywhere that they reported which methane calibration scale they were using. I'm assuming that it is the NOAA04, but it should be explicitly stated in any case.

Page 5482; line 1. Perhaps I'm misunderstanding this, but it says "The output of the firn model is shifted to lower concentration until the smallest root mean square difference to the measured EDML data is achieved." Do the authors actually mean they reduced the linear scaling factor for the INPUT to the firn air model until the root mean square difference was minimized? I'm not sure what is meant by shifting the firn model. Also, as a syntax correction, it should be "concentrations" not "concentration".

Page 5482; equations. Many of these variables are undefined in the text. What is mo? What is co? M? S?

Page 5483-5484. The parameterization of the tex component of their model is not sufficiently justified. They obtain a value of tex of 2 years and discuss that the reason for such a long interhemispheric transport term is that they do not lower the concentration of their NH record to represent the entire NH. I have reservations about this approach for the following reasons: 1.) All previous studies that I have seen have estimated an interhemispheric mixing time of $\sim$1 year. This includes Fung et al (1991), Chappellaz et al (1997), Kahlil and Rasmussen (1983), and more recently in Cunnold et al (2002) (there are probably many others too). All of these studies tune their transport with tracers such as 85Kr, SF6, and CCl3F. An interhemispheric mixing time of $\sim$1 year is a broadly accepted value and to use a value of 2 years is not sufficiently justified in the paper, nor is their tuning procedure documented. 2.) Sowers (2010) lowered the cn, but so did Chappellaz et al (1997) (who is not referenced in this part of the paper). I would say this is the "accepted" method of modeling the IPD and I would expect justification for why this method is wrong or inferior if the authors wanted to use a different method.

I do not expect that these model parameter choices will change the magnitude of the modeled methane emissions much or at all. However, it strikes me that they are using model parameters that are inconsistent with previous models and I would like to either see additional justification for their approach or just use a previously published model.

Another question I have about their model is they say the ratio mo/co = 1.45 Tg/ppbv (pg 5483 line 2) and they use this to convert concentration to mass. First of all I could not find this figure in Steele et al (1992) and second of all this is much different than that reported by Etheridge et al (1998) which used 2.767 Tg/ppb (from Fung et al (1991)) and Mitchell et al (2011) which used 2.844 Tg/ppb. Perhaps this is just representing $\frac{1}{2}$ the values used in Etheridge et al (1998) and Mitchell et al (2011), but this should be explained more clearly and should have a better citation (my apologies if Steele et al (1992) do report this, but I couldn't find it).

A final question/comment about the model. A particular set of transport terms was used for both glacial and interglacial time periods. I would like to see some discussion about how changing atmospheric transport may have affected the rIPD on glacial-interglacial timescales. This could presumably be a very important factor in interpreting the records and could be the reason for the very slight differences between glacial-interglacial differences in the rIPD. It might be unrealistic or unhelpful to try to model any of these changes with a simple two box model, but I would like to see a qualitative discussion about what the literature suggests atmospheric circulation changes to be (if there is any literature on this). If there is no literature on it then the authors should point out the need for future work on this topic.

Page 5485; line 2-4. This sentence indicates that boreal emissions control the isotopic composition of CH4, but this is not exactly true. Boreal wetland and tropical wetland emissions have a similar δ13C signature. Also, this sentence seems to have too many ideas in it and should be rewritten for clarity.

Page 5485; line 14. The cs/cs,ref curve in plot 6 looks pink to me, not red. Labeling it as red threw me off because the "pink" curve looks more red to me than the red curve.

Page 5485; lines 18-21. While the rIPD mean values have the trends that the authors indicate, the uncertainties are large enough to make this type of differentiation a bit suspect. For example, the rIPD estimates between 24-28ka are all not statistically different from the reference time period. The estimate at 21ka is lower, but DO2 is higher. So, is there really a robust argument that "the rIPD tends to be lower" than the reference time period from 21-28ka? Similarly, of the 4 rIPD estimates between 11-21ka, 3 of them are not statistically different than the reference time period, so is the statement that the "rIPD tends to be higher" robust? Perhaps what is needed here is just a little more verbiage acknowledging that while the rIPD mean values are higher or lower, the estimates are not statistically different from the reference time period.

Page 5486; lines 9-12. This idea that an ITCZ shift may change the volumes of the NH/SH boxes is interesting, but the implications for the IPD are not clearly spelled out. The authors indicate how it might affect the concentration in the boxes, but not how it

might affect the IPD. If there is a significant difference then presumably they can model it to quantify it.

Page 5486; lines 17. The reference to Singarayer et al (2011) is inaccurate. The ITCZ is not mentioned once in the main paper and in the supplemental material it only mentions that the ITCZ is correlated with 65N summer insolation when NH ice sheets are large and 30N during interglacials. In my view Singarayer et al (2011) find that subtle changes in the regional forcing of all methane regions combine to cause the 0-5ka increase in emissions.

Page 5486; line 21-23 & many other places. The terminology used here to refer to the Holocene is slightly confusing. Chappellaz et al (1997) have 4 time periods in the Holocene: 0.25-1ka, 2.5-5ka, 5-7ka, and 9.5-11.5ka. Throughout much of this paper the authors refer to the time period 0.25-1ka as "Preindustrial Holocene" but this is inaccurate because presumably the "Preindustrial Holocene" is any time period between 0.25-11.5ka. This should be changed to either "Late Holocene" or "Late Preindustrial Holocene". Further, the mid Holocene is typically referred to as $\sim$5ka, but this is right between the two time periods of 2.5-5ka and 5-7ka. I encourage the authors to just define what they mean by "Mid Holocene" and stick to that definition within the paper.

Page 5486; line 28. In the comparisons with the South American speleothem records, I would suggest that the authors consider the record from this paper: Wang, X. F., A. S. Auler, R. L. Edwards, H. Cheng, E. Ito, Y. J. Wang, X. G. Kong, and M. Solheid (2007), Millennial-scale precipitation changes in southern Brazil over the past 90,000 years, Geophys. Res. Let., 34(23), 5. This record is continuous over the CH4 IPD time period considered in this paper and shows much tighter anti-correlation with the Asian speleothems. Perhaps some of the discussion in the paper would change based on looking at this speleothem, but for now I will continue to review the IPD record compared only to the Kanner et al (2012) speleothem record as the authors have written the paper.

Page 5487; line 7-8. The uncertainties associated with the IPD and the rIPD show that the level at 21ka is not statistically different from the levels at 26ka, DO3 or DO4, so I disagree that the levels at 21ka can be labeled the "glacial maximum in the CH4 cycle".

Page 5487; line 14-28. There are no simple to interpret trends in either of the speleothem records around 21ka (for example Hulu does not have any trend at 21ka, but changes quite a lot between 15-20ka). My feeling is that since the IPD and rIPD records presented here are indicative of long time periods and are not a continuous record it is not especially helpful to discuss variability that is much shorter than the IPD time periods. Also, the authors note that we should be careful in comparing speleothems and ice cores due to chronology synchronization issues, but I would further argue that there is no simple relationship that could relate speleothems to ice core methane records on timescales shorter than millennial. This is in part because there is still not a widely accepted interpretation of what exactly speleothems represent (amount effect or seasonal effect). I have found this paper to be very illuminating:

Clemens, S. C., W. L. Prell, and Y. B. Sun (2010), Orbital-scale timing and mechanisms driving Late Pleistocene Indo-Asian summer monsoons: Reinterpreting cave speleothem delta(18)O, Paleoceanography, 25.

Page 5488; lines 3-5. The millennial scale speleothem variations (including Wang et al (2007) above) during the BA/YD are a little clearer than during the LGM and I think this section could benefit from a discussion about this topic (despite the caveats of the speleothems listed above).

Page 5488; lines 11-23. All 3 of these arguments have issues which should be addressed. 1.) The authors state that they need an INCREASED sn, but then state that the Afro-Asian drought actually caused a DECREASE in sn (lower latitude NH is still within the NH). This is a limitation of their present modeling framework which only has 2 boxes. If you only have 2 boxes then it is not technically possible to distinguish between NH tropical and NH boreal source changes. To do this the authors would need to fol-

low the modeling framework presented, for example, in Chappellaz et al (1997) which used a 3 box model. However, the drought also affects the tropical and SH portions of Africa which is consistent with a slightly decreased ss in the record, but the authors do not comment on this. 2.) It is unclear to me how a change in δ13C might indicate a change in the boreal source. In Fischer et al (2008) they argue that the increase in concentration must be biogenic and that the increasing IPD indicates that it must be boreal, but the δ13C by itself cannot distinguish boreal from tropical sources since they are both wetland sources and have a similar δ13C signature. In fact, perhaps an interesting thing to point out is how this study contradicts the findings in Fischer et al (2008). In Fischer et al (2008) they state "Considering the 50% reduction of atmospheric CH4 concentrations and the lack of an interhemispheric gradient in the LGM, a reduction of boreal wetland emissions is more likely." This study shows that there WAS an interhemispheric gradient in the LGM and so the increase in biogenic sources between the LGM and the BA must have occurred in both the boreal regions AND in the tropics. This is an important distinction and one of the main points from section 3.3, 5.1, the abstract and the conclusions but is not discussed here. 3.) It is not clear to me that benthic δ18O records should have a 1 to 1 relationship with boreal ice coverage and/or the northward migration of permafrost. Perhaps this is true but I would like to see a reference making this connection in this argument.

Page 5489; lines 12-26. The detailed discussion of fine scale features of the speleothems is interesting, but I do not think it adds anything to this paper. As I mentioned before, I would be cautious about interpreting the SH monsoon strength or the NH monsoon strength based on a single speleothem (Clemens et al (2010)).

Page 5490; line 3. It is not clear to me why the authors talk about the RATIO of Northern to Southern summer insolation (Ins/Iss) instead of just talking about the NH insolation? In figure 6 and 7 they just plot the NH insolation, not the ratio. They should either change the text to refer to just NH insolation or add the Ins/Iss curve to figures 6 & 7.

Page 5490; line 10. First, I actually can't tell when the maximum in Ins/Iss is because it is not plotted. Presumably the authors are talking about NH summer insolation maximum, but that is not until ∼9ka (not ∼14ka as they mention in the text) and the maximum in the rIPD is at 16ka, not 14ka! I actually don't know why the authors mention 14ka at all here.

Page 5490. The author's interpretation is not consistent with the data here. On lines 11-12 they argue for a long term trend (ignoring the data point that they don't like at 4ka) when the data error bars all intersect the reference value, so the trend cannot be statistically different from zero (relative to the reference). On lines 12-14 they do the same thing for the time period from 30-20ka and again have the same problem, all the data is not statistically different from the reference value except for DO2 (which they don't want to include in their trend) and the one data point at 21ka. To me the more important finding should be exactly the opposite of what they are claiming: there are NO statistically significant trends associated with orbital scale changes in insolation, contrary to the Singarayer et al (2011) model. This would be clearer if the authors had plotted their error bars on figure 7 and included DO2 in their time series, or not plotted a line at all, or the authors should exclude ALL the DO events from their time series which would make sense because Singarayer et al. (2011) did not claim to simulate millennial scale events. If they did this it might be clear that the Singarayer et al (2011) estimate of the rIPD for 20-30ka is too low. This would be an important finding to report because modeling studies can use a relatively constant rIPD as a modeling target, and the variations in the Singarayer et al (2011) record may be too large.

After re-reading the abstract and conclusions, it seems that those parts of the paper stress a relatively constant rIPD and this portion of the paper is inconsistent with the abstract and conclusion.

Page 5491-5492. Some of the conclusions may change based on the above comments, particularly the last part about the monsoons. The Abstract is similar. I'm sure the authors will do a good job updating the abstract and conclusions based on the above comments.

Page 5505 Figure 5. I think the labels inside the figure area (tao = 10.1 yr and tex = 2 yr) are supposed to be switched between the upper and lower panel.

Page 5508 Figure 7. Refer to the comment above. The error bars should be plotted. Also, DO2 should be included in the time series, or all the DO events should be excluded from the time series, or there should be no line at all.

Technical Corrections

Page 5473; lines 12-15. The sentence which starts with "Wetland CH4 production..." is confusing because it is relating the factors which affect methane emissions and the correlation of satellite observations to temperature and precipitation. These two things (factors controlling emissions and observed correlations with emissions) should be in separate sentences for clarity.

Page 5487; lines 2-3. The sentence starting with "In line the..." is confusing and should be re-written for clarity.

Page 5487; line 25. Also on Page 5492 line 5. Modern emissions are 2.5 times the Late Preindustrial Holocene, not "twice as large". This could be changed to "more than twice as large" but it would be nice to just explicitly say 2.5 times as large.

Interactive comment on Biogeosciences Discuss., 9, 5471, 2012.